Communications.

Novel Discoveries in Aerial Propulsion.

To the Editor of the Scientific American :

I recently picked up the *Galaxy* for April, 1872, and my attention was drawn to an article entitled, "Flight a Screw Propulsion." Glancing over it, I came to the following: "In 1867, Dr. J. Bell Pettigrew, of the Edinburgh University, before the Royal Institution of Great Britain, first propounded the now celebrated theory of the figure of 8 wave motion of the animal wing, and this has since been confirmed by the observations of Marcy."

"Pettigrew himself, before giving his conclusions to the public, had, with commendable caution, subjected them to careful verification."

"He continued his researches, and in 1868 published an elaborate memoir on the mechanical appliances by which flight is attained in the animal kingdom."

"During the wing's vibrations, it twists and untwists, so that it acts as a reversing, reciprocating screw, and resembles the blade of an ordinary screw propeller."

"The twisted configuration of the wing, and its screwing action, are due to the presence of figure of 8 looped curves on its anterior and posterior margins," and "Dr. Pettigrew has derived his ideas of the structure and movements of wings from careful anatomical study, and the most patient observation and experiment with winged animals themselves; and in view of these facts, he does not hesitate to avow the opinion that a thorough knowledge of this branch of animal mechanics will yet give man the power of artificial flight."

At considerable length the remarkable discovery by Pettigrew is entered into, and would seem to have been the result of years of observation, and promises still to be its object until man shall fly away on the strength of it. But it is evidently supposed by the great scientist that the mainspring of flight not only consists in the figure of 8 described by the extremity of the wing, but involves the necessity of particular muscles and sinews especially provided to give it the required twist.

In the first place, so far as regards the novelty of the idea that flight is accomplished by the screw propulsion of the wing, he has but to find himself forestalled by the SCIENTIFIC AMERICAN (in 1853, I think somewhere about October), wherein are two engravings of the propeller for which a patent was granted to Charles T. P. Ware, consisting of two elastic blades or wings, adjusted to an oscillating shaft, and which have their submerged reciprocating sweeps in an arbitrary plane perpendicular to the line of propulsion, forming a screw at each sweep. This arrangement, the inventor says that he adopted from his closest observations of the wing action in the swiftest of birds and insects, as well as the two-bladed tail of the East Indian swordfish. Indeed, the wings of the dragon-fly are so fixed in that position that they cannot be actuated in any other way. The idea, then, of screw propulsion in the animal wing would not seem to be quite so original with Dr. Pettigrew as he might have supposed, and to which he lends such weighty importance as a "discovery" long held secret until verified!

In conclusion, the screw action is not due to the figure of 8 configuration, the latter not being a cause, but an effect or consequence, of the propulsive movement of the wing. The very fact of the blade, or wing, being elastic, with the forward edge rigid and tapering, and the sweep forced rapidly and directly from upward to downward and *vice versa*, it could not impinge on the resisting medium (air or water) without describing at the tip that double loop from the points where it takes its start for every return stroke. This latter discovery, which is necessarily embodied and referred to as a feature demonstrated in practice, in Ware's patent, is therefore not only no novelty from the Doctor who is said to have first propounded the now celebrated theory, but shows that no mechanical appliances need be resorted to by inventive genius to twist the action into figures of 8, since, whether that be the secret of the motive force or not, it is already supplied by the simple action of the wing arbitrarily confined to a plane perpendicular to the direction of flight.

It therefore appears that, in the matter of the two great foregoing startling novelties, the SCIENTIFIC AMERICAN is at least about fifteen years ahead of Pettigrew and the Royal Institution of Great Britain! LECTEUR CONSTANT.

Aeronautics.

To the Editor of the Scientific American :

I have noticed in some of your recent issues several articles on flying machines. The subject is one in which I have taken a great deal of interest; and as the conclusions at which I have arrived differ altogether from those of your correspondents, it is just possible they may give a new direction to the discussion.

I believe the invention of a machine, to fly by acting mechanically on the air, as birds do, is simply impossible if the machine, with its load, weighs more than 50 or 60 lbs. I do not say that a machine of any weight may not be constructed which shall be just a little heavier than the air displaced, and then the machine may be raised mechanically by acting on the air; but such a machine will, for reasons which follow, be little, if at all, better than a balloon. That which enables a bird to fly is the support which the pressure of the air gives to the bird's body. This support depends, I think, on the proportion between the weight and the surface exposed to the air. If the size of a bird is increased, all other things being equal, the weight increases in a greater ratio than the surface exposed to the air; so that, if with a certain amount of wing area and muscular power a bird weighing 10 lbs. could fly well, and his weight were increased to 30 lbs., with muscular power and wing area increased in the same proportion, he could not fly at all. Or if an eagle grew as big as an elephant, he could no more fly than the elephant. Let us suppose that a bird of 10 lbs. weight is a perfect flying machine. Our object is to increase the size of the machine and keep the same perfection of parts. If the weight is doubled, keeping the same proportion of all the parts and using the same material, we will find that the muscular power has not quite doubled, and the supporting surface exposed to the air has not increased in anything like the same proportion; so that a limit is soon reached where the machine ceases to have any power of flight, and that limit, where muscular force is the power used, I take to be about 30 lbs. This accounts for the fact that all the largest birds are not fliers. The ostrich, the emu, and the moa ceased to be flying birds as soon as they grew beyond a certain size, which size was determined by the proportion between their weight and the surface exposed to the air. Geology also shows that, while mammals and reptiles grew in past ages to enormous sizes, no flying animal ever appeared much larger than those now existing.

In this way only is it possible to account for the fact that small particles of iron or steel dust will float for a long time in the air. Of course each particle is as much heavier in proportion than the air as if it were a solid cube several inches in diameter. This also accounts for the fact that the wing area in small birds is not nearly so large, in proportion to weight as in the larger birds; and the wing area in proportion to weight is still further diminished in many insects, such as the common bee and many of the beetle tribe. I have seen some small animals in this country, such as the opossum and the rock wallaby, fall 50 feet on a solid rock without injury; and this first set me speculating on the why. A bullock falling under the same circumstances would have been crushed, bones and all, to a shapeless mass; and yet the wallaby is not more strongly made than the bullock. I have stated my views as shortly as possible, and if I have not made them plain to general readers, I trust some mathematician among your correspondents may take the matter up and show that according to well known mathematical laws flying (as birds fly) is impossible for men.

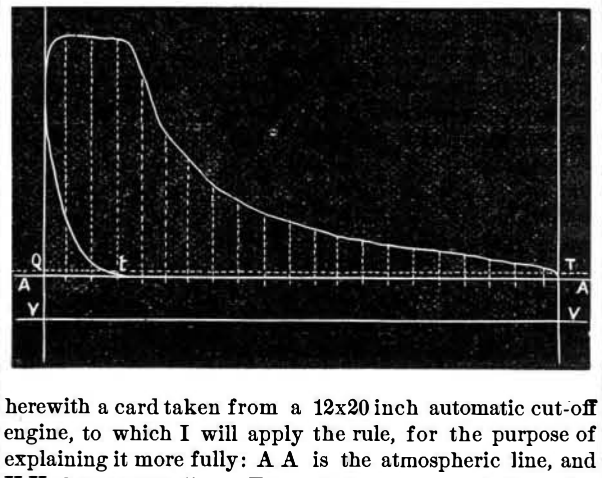
Murrurundi, New South Wales. W. E. ABBOTT.

Water Evaporated through Engines.

To the Editor of the Scientific American :

I have before me the circular of an engine manufacturing company, in which the proprietors explain their method of computing the water consumption per horse power per hour, of any engine, from its indicator card alone. The method is as follows: "Divide the constant number 859,375 by the mean effective pressure of any diagram, and the quotient by the volume of its total terminal or exhaust pressure, the result will be the theoretical consumption in pounds of water per horse power per hour." "The constant number used is the piston displacement for one hour, in lbs. of water, of an engine which would develop one horse power with 1 lb. pressure of water instead of steam. Then, with pressures of more than 1 lb., the amount required would be as many times less as the pressure was greater than 1 lb.; and when steam is used, the amount would be as much less as the volume of the steam at the pressure at which it is released is greater than an equal weight of water. The volumes of the pressures are taken from Forney's "Catechism" and Roder's "Handbook."

It is easy to see that if the steam in the cylinder followed, strictly, Mariotte's law of expansion, and if the valve and piston fittings were perfect, this would be a very accurate, as it is a simple, rule to go by; but as indicator cards give us but very little clue to the amount of leakage and condensation, a considerable amount of water will pass through the engine, for which the rule makes no allowance. Indicator cards are of great value in determining the initial, mean effective, and terminal pressures, the back pressure, the cushion, whether by compression or lead, the point of cut-off, and the relative economy of different engines, aside from leakage and condensation. As so much depends upon the construction of the engine, it seems to me that no definite rule can be given for arriving at a near result. I inclose



herewith a card taken from a 12x20 inch automatic cut-off engine, to which I will apply the rule, for the purpose of explaining it more fully: A A is the atmospheric line, and V V the vacuum line. The initial pressure is 72 lbs.; the mean effective pressure is $25\frac{2}{5}$ lbs.; and the total terminal about 16 lbs. (measuring from vacuum line). The cut-off is effected at about 16 per cent of the stroke. Applying the rule to this card, we have $859,375 \div 25\frac{2}{5} = 33,834 \div 954 = 35{\cdot}46$ lbs. of water per horse power per hour (954 being the volume of the 16 lbs. pressure).

When cushioning by compression is employed, a part of the steam is saved; so that, when greater accuracy is desired, we proceed thus: "Multiply the result obtained by the rule by the length of the dotted line, T, *t*, and divide the product by the length of line, T, *a*." I would like to hear from others on this subject.

Hinckley, Ohio. W. A. MUSSEN.

Decomposition of Water by Sodium Amalgam.

To the Editor of the Scientific American :

In a recent number of your valuable paper, my attention was drawn to the article by Professor Merrick entitled "Mortification and Water," taken from the *American Chemist*. As I have repeated the experiment a number of times, and have had precisely the same experience in breaking the glass vessel, I at last hit upon the method of forming an amalgam of the sodium with mercury, which not only makes the decomposition of the water to take place slowly, but, by increasing the weight of the sodium, may be conveniently kept in a small capsule of porcelain at the bottom of the jar, and the minute bubbles of hydrogen rise rapidly through the water, thus increasing the beauty of the experiment. A wire cage may be also employed for confining the sodium; and such an instrument, furnished with a handle, can be bought in our stores where philosophical and chemical apparatus are sold. A tea ball, made of wire gauze, and intended to keep the leaves of the tea together in the pot, may also be pressed into service; but of all the plans proposed I decidedly prefer the amalgam one, which will also answer, when thrown into a solution of ammonium chloride, for forming that remarkable compound which, when seen for the first time, excites so much wonder, namely, the ammonium amalgam.

Philadelphia, Pa. ISAAC NORRIS, M.D.

[For the Scientific American.]

EXPERIMENTS WITH LOCUST EGGS, AND CONCLUSIONS DRAWN THEREFROM.

BY PROFESSOR C. V. RILEY.

There are many questions respecting the manner in which the eggs of the Rocky Mountain locust are affected under different conditions, which are of intense practical interest, and which are frequently discussed with no definite result being arrived at, or no positive conclusion drawn. Such are, for instance, the influence of temperature, moisture, and dryness upon them; the effects of exposing them to the air, of breaking open the pods, of harrowing or plowing them under at different depths, of tramping upon them. Everything, in short, that may tend to destroy them or prevent the young locusts hatching, is of vital importance. With a view of settling some of these questions, and in the hope of reaching conclusions that might prove valuable, I have carried on during the past winter a series of experiments which will be reported in detail in my 9th report, and the conclusions drawn from some of which I give you herewith:

Nine experiments, to test the

EFFECTS OF ALTERNATELY FREEZING AND THAWING,

showed that: 1st, the eggs are far less susceptible to alternate freezing and thawing than most of us, from analogy, have been inclined to believe. Those who have paid attention to the subject know full well that the large proportion of insects that hibernate on or in the ground are more injuriously affected by a mild, alternately freezing and thawing winter, than by a steadily cold and severe one; and the idea has quite generally prevailed that it was the same with regard to our locust eggs. But if so, then it is more owing to the mechanical action which, by alternate expansion and contraction of the soil, heaves the pods and exposes them, than to the effects of the varying temperatures. 2nd, that suspended development by frost may continue with impunity for varying periods, after the embryon is fully formed and the young insect is on the verge of hatching. Many persons, having in mind the well known fact that birds' eggs become addled if incubation ceases before completion when once commenced, would, from analogy, come to the same conclusion with regard to the locust eggs. But analogy here is an unsafe guide. The eggs of insects hibernate in all stages of embryonic development, and many of them with the larva fully formed and complete within. The advanced development of the locust embryo, frequently noticed in the fall, argues nothing but very early hatching as soon as spring opens. Their vitality is unimpaired by frost.

A series of sixteen experiments, to test the

INFLUENCE OF MOISTURE UPON THE EGGS,

establish a few facts that were somewhat unexpected. I give one of the experiments as a sample. The insect is a denizen of the high and arid regions of the northwest, and has often been observed to prefer dry and sunny places, and to avoid wet land, for purposes of oviposition. The belief that moisture was prejudicial to the eggs has, for these reasons, very generally prevailed. The power which they exhibit of retaining vitality and of hatching under water or in saturated ground is, therefore, very remarkable—the more so when viewed in connection with the results obtained in the succeeding experiment. That the eggs should hatch after several weeks' submergence, and that the young insect